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of the cage, or a vague idea of the food resulting in the masticating before the prey is caught) determines any part of the activity, we should be obliged to assert then not a reversal of the causal order, but rather a non-causal sequence to the extent to which that factor entered into the determination of the process. I am not here contending for any such hypothesis; I merely desire to point out an intelligible conception of the expression, the response to a situation in advance of the existence of that situation. On any other basis such phrases as dissatisfaction and striving employed in connection with the description of what is held to be a series of physical events, even in the capacity of inefficacious correlates, have no significance nor excuse for being. Variability and selectiveness in response, conceptions taken over from mental life, are rendered unintelligible when made descriptive of strictly mechanical series. From the standpoint propounded there can be no variation of response; every reaction is as necessary as every other. Picturesqueness here tends toward confusion and does not assist in clarification.

If the above considerations concerning the significance of a description based upon the point of view of the observer hold, then the basis for assimilating organic purpose to conscious purpose disappears. There is no meaning in the reversal of a causal order if a physical explanation obtains. There is no place for a concept of preparation or anticipation employed in a sense which permits it to serve as a basis for agreement between the two divergent orders of events distinguished as mental and organic, the latter ultimately reducible to a special type of physical process. If the assumption of psychophysical parallelism proves not so clear and satisfactory in its developments as it may appear to be, is it inevitable that an hypothesis diverse from this necessarily retards the study of the brain and nervous system?⁷

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"DUALISM AND ANIMAL PSYCHOLOGY:" A REJOINDER

PROFESSOR WASHBURN'S reply to my recent article *Dualism in Animal Psychology* raises so clearly and insistently the fundamental issues in dispute between the dualist and the behaviorist, that a rejoinder seems imperative, the more so since my article failed to make clear in certain matters the real point of my criticism.

⁷ E. B. Holt, *The Concept of Consciousness*, p. 308. "Now in attempting this deductive account of consciousness, I have had one prime purpose in view, and that is to free once and for all the study of the physiology of the brain and nervous system from its present and retarding association with metaphysics."

As I understand it, the issue is briefly this: Is psychology properly concerned with a class of phenomena (conscious processes) which are observable only by one person? Must therefore the psychological study of animals and of one's fellow-men rest, first, upon the argument from analogy, and, second, upon the capacity to reconstruct imaginatively the mental processes of the animals or human beings in question?

Behaviorism, as a scientific theory, and not a metaphysical doctrine, is not concerned with the question whether or not there be conscious processes which are hidden from all but one. Its contention is merely that if there be such processes they can not by the very nature of the case be objects of scientific study. For it is an essential condition of scientific investigation of any phenomenon that observations made by one individual shall be verifiable by others. Otherwise indeed a phenomena is not even identifiable. This was the point of my argument that psychological phenomena investigated experimentally "become in effect functions of the factors constituting the standardized conditions of the experiment." Professor Washburn's reply, that the dualist may admit this without affecting his claim that the phenomena are in themselves observable only by the subject, does not meet the real objection, namely, that it is only *as* functions of standardized conditions that they can become objects for science.

Suppose that the problem to be investigated is the determination of minimal changes in grays. The standardized conditions of the experiment include constant lighting, distance of observer from stimuli, time and order of observations, *etc.*, and finally, the use of standardized black and white paper. Let us admit with the dualist, for the sake of argument, that what the subject is observing is a visual sensation-quality which is private and incommunicable. The essential fact remains that the observations can only be described in such terms as: "observations of revolving discs of so many degrees black and so many degrees white," *etc.* Furthermore, earlier and later series of observations can be correlated with each other as observations of the "same" phenomena, only in so far as the phenomena are described in similar objective terms. In other words, the dualists' assumption of the private and incommunicable character of the phenomena under investigation is wholly inoperative for the scientific procedure in question. It affects neither method nor result. What is being investigated is the subjects' capacity for discriminating differences in objective stimuli.

The same point is involved in Professor Washburn's comment: "Nor would the dualist realize why Dr. de Laguna needed to occupy a page in showing that in actual procedure and in results

the studies of a dualist and of a behaviorist in the field of comparative psychology are identical. Since we can obtain no introspection from animals, such a statement would appear to be self-evident: it is the interpretation of results that differs for the two types of workers." But it is precisely the scientific value of such additional "interpretation" which is in dispute. My contention was that so far as *scientific* procedure and *scientific* results are concerned the dualist and the behaviorist are practically at one, and that "just in so far as the dualist claims to infer from the facts of behavior the existence of an inner order of being related in an inscrutable way to those facts, he is stepping outside the bounds of scientifically verifiable hypothesis and entering upon purely metaphysical speculation in the bad sense of the term." This contention, which to me seems the essential one, together with the charge that the dualists' "interpretation" involves, on Professor Washburn's own showing, an appeal to supernatural insight, has been entirely ignored in her reply.

The issue between the behaviorist and the dualist, upon which the whole controversy turns, is, I believe, the nature and status of introspection. It is here that the real strength of the dualist seems to me to lie, as is brought out in Professor Washburn's reply. It is so obvious on the one hand that there are things, like the pain in my tooth or the pressure on the back of my hand, that are directly observable by me and by no one else in the world, and with which psychology is clearly somehow concerned, that the position of the dualist seems inevitable. On the other hand, it is so obvious that the pain in my tooth and the pressure on my hand are not themselves modes of behavior, that the alleged proposal of the behaviorist so to classify them seems sheer perversity. If behaviorism is to be made a reasonable doctrine in the eyes of the dualist it must take account of these facts. Advocates of behaviorism have usually failed I think, to distinguish properly between the behavioristic status of such commonly recognized psychological phenomena as "sensation" on the one hand and "emotion" on the other. The claim that the study of emotion is a study of a type of behavior is plausible enough, but the claim that the study of sensation is a study of modes of behavior is open to obvious objections. "Red" is not a set of reactions in the body but a directly observable somewhat; so also are "pressure" and "pain." But to recognize this is by no means to accept dualism. It is quite open to behaviorists to admit the possibility of directly observing these phenomena, and of course many behaviorists, notably the neo-realists, have maintained this position most vigorously.

Let us admit this claim. Let us admit also that these and other

similar phenomena come within the field of psychology, or at least within the scope of introspection, in so far as they are directly observable by one person only. Psychological introspection is then to be distinguished from ordinary "objective" observation just because it is the observation of that which is essentially private and incommunicable. To the all-important problem which is thus raised: how introspection, which is by definition a sort of observation unverifiable by others, can yet possess scientific value, the only solution I know is that offered by behaviorism, *viz.*, that introspection has such a value only in so far as the introspective observations of the subject are treated as *responses*, and not as statements of observed facts. In other words the introspections are data for the psychologist, as the flight of the bee is for the naturalist, digestion is for the physiologist, or the burning of coal for the chemist. This solution is, I believe, theoretically sound, and it accords moreover with the actual procedure of the experimentalist. The real scientific observer in the psychological experiment is not the *O* but the *E* of the experiment. The series of introspections is a series of responses given by the *O* under the conditions of the experiment, and observed and interpreted by the *E*.

That introspection is a peculiar type of response which needs careful analysis in order to distinguish it properly from other types of response is of course true. I must frankly admit that no behavioristic discussion of it which I have yet seen seems at all adequate. But I do believe that behaviorism offers the only promising theoretical basis for a fruitful analysis of the nature and limits of introspection.

It remains to say a few words in reply to Professor Washburn's question as to the possibility of a non-mechanistic behaviorism. The problem is of course far too large a one to be properly discussed within the limits of this rejoinder. It would seem, however, that it is the assumption of the possibility of a mechanistic behaviorism, *i. e.*, an exhaustive description and explanation of the phenomena of human and animal behavior in terms of physical science, which, in view of the actual achievements of biological science is in need of justification. But however that may be, the terms in which behavior is actually describable to-day are very far from being exclusively physical, or even physiological. The claim that physical or physiological terms are the only ones in which an objectively valid description of behavior can be given, would appear to me, I confess, nothing short of metaphysical dogmatism, and for this reason I doubt whether I have rightly understood Professor Washburn's position. When she writes: "Dr. de Laguna seems to mean . . . that there exists a form of behavior which is not either nervous action or mus-

cular action. I can not guess what behavior, so interpreted, is," my doubt increases. Professor Washburn here ignores a distinction which to me appears of cardinal importance, that between physiological process on the one hand, and behavior on the other. Of course in one sense there is no behavior that is not nervous or muscular action, just as, for example, there is no digestion that is not chemical action. The physiologist classifies a given process as "a digestive process" not on the basis of its chemical character, but because it bears a certain type of relationship to other processes making up the life cycle of the organism in question. The same chemical process occurring in a different organism might not be a digestive process at all because it would not occupy an analogous place in the life cycle of that organism. If we compared the digestive processes of a jellyfish and a rat we might conceivably find no chemical identities at all. Physiology, in short, is primarily the analysis of the internal bodily processes with reference to the fact that they constitute a vital economy. It is the exhibition of a schema, a type of systematic relationship. The schema once made out, the detailed investigation of how, in each distinct species, the various processes actually play their parts in the schema, depends on the use of chemistry and physics. But the use of chemical and physical categories is distinctly subsidiary, albeit indispensable, to the actual procedure of physiology.

In a perfectly analogous way the use of physiological categories (as well as those of physics and chemistry) is subsidiary, albeit indispensable, to the procedure of the behaviorist. For the external behavior of the living being also constitutes a life cycle, an economy analyzable into different factors from those found by the physiologist. A certain response is classed as "play," or a "fear response," not because it consists of certain specific muscular contractions or nervous processes, but because in the individual in which it occurs it occupies a specific place in the larger vital economy which constitutes his behavior. The task of the behaviorist, as I conceive it, is primarily, like that of the physiologist, the exhibition of the complex activities of the living being as a systematic economy. The schema which the behaviorist has to exhibit is vastly more complex than that of the physiologist, since the relationships constitutive of the factors of the schema include relationships with factors of the environment, not exceptionally as in the case of physiology, but essentially and systematically. Moreover such factors in the environment are themselves factors in the schema of behavior. It is for this reason that the economy which the behaviorist has to investigate forms the subject matter of a distinctive science—psychology.

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